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pose the brightness of the limb to be 0.07. Ten times this brightness would be expressed by 0.70. The *average* brightness of *Jupiter* is expressed by 0.62, and hence it is clear that our estimates are not extravagant.

From the table above, it follows that much of *Jupiter's* surface is not more than three or four times as bright as the limbs; or, numerically, three or four times  $0.07 = 0.21$  or  $0.28$ . On a background of brightness 0.21 or 0.28 a satellite of brightness 0.22 (Satellite I) or 0.27 (Satellite II) would usually be lost. There is not sufficient contrast to make it appear either as dark or bright. If, however, any of the satellites chanced to fall on the brightest regions of the planet (brightness = 0.70 or greater), they would certainly appear as dark bodies. I take it, therefore, that the only reason why dark transits of II are not recorded (so far as I know), is because they have not been looked for with the proper instruments and precautions.

The preceding explanation appears to me to account for the facts as well as the meager numerical data will allow, and to have an advantage in that it calls for no assumptions whatever. No collection of past observations of dark transits can throw any material light on the question; but future observations of phenomena of the sort will be of value, if they give numerical estimates of the brightness of the backgrounds on which the satellites are projected.

E. S. H.

#### NOTE ON THE OPPOSITION OF *MARS*, 1890.

The very severe weather of 1889-90 lasted unusually late into the spring, and the season of excellent nights hardly began before late July or early August. By this time *Mars* was too distant from the earth and too low in the west to be well-observed. All available opportunities were utilized in the early parts of the opposition, and the planet was regularly followed during June, July and August. Experiments were tried with colored glasses, with diminished apertures, etc., all with small success. Many photographs of *Mars* were made, but none of any real excellence.

Drawings of the planet were made on April 3d, 9th, 12th, 26th, 30th, May 3d, 11th, 15th, 18th, 21st, 25th, June 5th, 6th, July 6th, August 5th and 6th. In making these drawings three observers usually took part. E. S. H. and J. E. K. always saw the *canals* as dark, broad, somewhat diffused bands. In bad vision they were drawn in this way by J. M. S. also. Under good conditions, however, the

latter observer described them as narrow lines, a second of arc or so in width.

On April 12th J. M. S. saw two of the canals doubled. It may, therefore, be said that the observations of Professor SCHIAPARELLI have been verified by this observer, both as to the narrowness of the canals and as to their duplication. The positions of most of Professor SCHIAPARELLI'S canals have been verified by some one of us.

The mystery still remains why two observers should agree in their own observations, and should disagree with a third and with the discoverer of these phenomena. A remark by Mr. KEELER (*Publ. A. S. P.*, vol. II, p. 165), with regard to the color-correction of the large telescope and its effect on the seeing of faint planetary markings, throws a little light on this question, but fails to explain how three observers of experience, viewing the same object, with the same instrument, at the same time, can obtain such differing impressions. A review of the observations of *Mars* in 1888 shows that the same observers differed in somewhat the same manner at that time; and an examination of the drawings of E. S. H. from 1875\* to 1890 shows that this observer has always drawn the canals in one and the same manner. Letters from Professor SCHIAPARELLI say that he has succeeded in obtaining twelve or fifteen excellent drawings during 1890, which confirm his previous discoveries, and which even extend the area over which the doubling of the canals takes place.

E. S. H., J. M. S., J. E. K.

#### THE FUTURE OF STELLAR PHOTOGRAPHY.

[Extract from a Letter written in 1857 to WM. MITCHELL by G. P. BOND.†]

"As far as I am informed, the attempt to photograph the fixed stars by their own light has been made nowhere else, up to the present date; the rumor of a daguerreotype of a nebula made in Italy some years since was unfounded.

"About seven years since (July 17th, 1850) Mr. WHIPPLE obtained daguerreotype impressions from the image of a *Lyræ* formed in the focus of the Great Equatorial, and subsequently from *Castor*, thus establishing a simple but not uninteresting fact—the possibility of such an achievement. On these occasions a long exposure of one or two minutes was required before the plate was

\* See *Mémoires couronnés etc. publiées par l'Académie Royale de Belgique*, vol. XXXI, figs. 1-5 (1875 June-August).

† Professor GEORGE PHILLIPS BOND was then Assistant in the Harvard College Observatory. The extract given above was kindly communicated by his daughters.